ANALYSIS

Edited by Bernard Mayo, with the advice of A. J. Ayer, R. B. Braithwaite, Herbert Dingle, A. E. Duncan-Jones, P. T. Geach, C. A. Mace, A. M. MacIver, and H. H. Price

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ANALYSIS COMPETITION—TWELFTH "PROBLEM"

The twelfth "problem" is set by Professor J. L. Austin, of Oxford, and is as follows:

"All swans are white or black." Does this refer to possible swans on canals on Mars?

Entries (of not more than 600 words), accompanied by stamped, addressed envelope or international stamp voucher if the return of MSS. is desired, should be sent to **The Editor of Analysis by Friday, December 20th, 1957.** *No entries should be sent to Professor Austin.* Contributors may submit entries either under their own names or under a pseudonym. A report, with any winning entries, will be published, if possible, in ANALYSIS for March, 1958.

THE EDITOR.

NEW LIGHT ON THE LIAR

By Y. BAR-HILLEL

TEN years ago, I published a discussion note[1] in which I criticized a certain attempt at solving the antinomy of "The Liar" without departing from the point of view of traditional logic. Though I still believe that my criticism there was basically correct, I no longer think that my own way of looking at the problem at that time is the only proper and fruitful one. I hope that my change of mind will be of some general interest.

My motive for writing this paper was supplied by an article written by John G. Kemeny for the 1957 edition of the *Encyclopedia Britannica*.[2] It is perhaps not quite up to customary academic standards to write a paper for a technical journal in reaction to an Encylopedia article, but in this case I hope my breach of custom will be forgiven. Kemeny's article is outstanding in its amazingly successful fusion of precision, clarity and readability and is in my opinion fully entitled to the claim of presenting an authoritative up-to-date description of (logical) Semantics.

There is only one point in which I find fault with Kemeny's presentation: his treatment of the Liar paradox and the consequences he draws from it. The view he presents is probably quite typical for many modern logicians, and I myself adopted it ten years ago. Let me quote:

[1] "The Revival of 'The Liar'", *Philosophy and Phenomenological Research*, Vol. VIII, 1947, pp. 245–253.
[2] "Semantics as a Branch of Logic", *Enc. Brit.* (1957) Vol. 20, p. 313.

> The next sentence is true. The previous sentence is false.
>
> If you consider either one of the foregoing sentences, you note that it is a perfectly simple and clear sentence of the English language. And yet the two sentences together constitute a paradox; they are a version of the ancient paradox of the liar. A (declarative) sentence must be either true or false, and cannot—of course—be both. Let us suppose that the first sentence is false. That means that the second sentence is not true. But to say that " The previous sentence is false " is not true is equivalent to the assertion that the previous sentence is not false. This itself is not a paradox; from the assumption that the first sentence is false we have derived a contradiction, which shows only that the assumption was wrong. Hence *the first sentence is not false*. That implies that it is true, which means that the second sentence is true. But if " The previous sentence is false " is true, then *the first sentence is false*. We have now derived two contradictory results using only the rules of logic.

After presenting in an equally clear and precise fashion Grelling's antinomy, the author concludes:

> The above paradoxes show that English allows the derivation of contradictory conclusions by means of the rules of logic, i.e. that English is inconsistent. Since it can be shown that in an inconsistent system anything at all, true or false, can be proved, we have to conclude that ordinary English is a language not suitable for logical arguments. And it is not the case that some simple trick will remove this inconsistency.

It is ironic that Kemeny tries to show that ordinary English is a language not suitable for logical arguments by offering us a fine specimen of a logical argument presented in ordinary English. (But I would not like my *argumentum ad hominem* to be taken too seriously).

Now, though Kemeny's argument seems to be perfectly valid, is it really sound? Is it really the case that " a (declarative) sentence of ordinary language must be either true or false "? What about ' It is raining ' or ' I am hungry ', to mention only the most simple and obvious counter-examples? With regard to these sentences *qua* types it is nothing short of ridiculous to regard them as being either true or false. But what about these

sentences *qua* tokens? (Kemeny has clearly sentence-tokens in mind: only of a certain *token* of the sentence ' The next sentence is true ' does it make sense to ask for its truth-value.) On another occasion,[1] I have tried to show how misleading it is to regard even tokens of sentences whose reference is context-dependent as being true or false (though I would now want to put my argument in different terms). But instead of repeating the argument, it should suffice to point out how silly it sounds to ask for the truth-value of a declarative sentence (-token) like ' I want you to close the door ' or ' I hereby declare the meeting open '.

All this is of course commonplace in Oxford for many years. Truth and falsity, according to the view prevalent there, apply directly only to *statements* and not to (declarative) sentences. And a statement can, but need not necessarily, be made by uttering a declarative sentence; it is made quite often by uttering a non-declarative sentence or even a non-sentential expression, and occasionally by nodding or through some other non-linguistic device. (Nor, of course, are all declarative sentences uttered in order to make statements.) It would be pedantic to disqualify *all* usages of the form '——is true', where the blank is filled by some sentence or by the words 'the next sentence' or the like; but such usage, according to the view presented just now, is always derivative and can be definitely misleading; and any philosophical generalisation to the effect that all declarative sentences must be either true or false is most definitely wrong.

But could not the version of the Liar paradox chosen by Kemeny be reformulated in statement terminology? Let us try. Assume that John is uttering in an appropriate voice the following two sentences one after the other: " The statement I am going to make by uttering the next sentence is true. The statement I made just now by uttering the previous sentence is false ". It should be clear that on the assumption that John, by uttering these two sentences, made two statements a paradox immediately arises, if we grant that *a statement must be either true or false*. But we are perfectly free to conclude that, despite appearances, no statements were made by uttering these sentences. Indeed, this conclusion is reasonable on reflection, quite independently of the argument through paradox. I for one would have judged so, after some consideration, even if John had said ' true ' in the second sentence, instead of ' false '. (All

[1] " Indexical Expressions ", *Mind*, Vol. LXIII, 1954, pp. 359–378.

this has, of course, been said *in essence* many times before, but I believe that the specific terminology used here is of decisive importance.)

Whether a given sequence of English words constitutes a sentence or not, is a question that—after certain qualifications and refinements—should be answerable on the basis of a list of formation rules which English grammarians should in principle be able to produce. There are even good reasons for thinking that the answer could be given by a mechanical procedure. (To use, for once, highly technical terminology, the term ' sentence in English ' should be " definite " in Carnap's terminology of the *Logical Syntax*, or the set of English sentences " generally recursive " in the customary post-Goedelian terminology.) But there is no reason to expect that the same should hold for ' statement '. It is, on the contrary, quite obvious that no general procedure could exist for determining whether a statement has been made. Examining the sentence used is certainly never quite enough, though occasionally not much more might be required. But for present purposes it is, fortunately, not necessary to go into the extremely difficult problem of determining what a statement is.

My " solution " of the Liar paradox—and it is easy to see that it applies also to the other known versions of it—should by no means be regarded as a defence of traditional logic. It is rather an attack against whoever, traditionalist or modernist, misdescribed the uses of ordinary language by stating, or implying, that declarative sentences must be either true or false (myself of years ago included). Nor does the proposed change of attitude to the concept of truth in ordinary languages commit us to any change of view about the concept of truth in systematized languages. There, a semantic truth concept is fully appropriate and the problem, how to safeguard these languages against the semantic paradoxes, is a serious one. But in the case of ordinary languages, the semantic truth conception loses its point, once we decide to regard truth as a property[1] of statements rather than of sentences. Paraphrasing Tarski[2] himself, we may say that *the attempt to set up a semantical conception of the term ' true sentence ', applicable to ordinary languages, is misguided.* Tarski's famous criterion for the adequacy of a definition of truth for ordinary languages becomes, in our terminology, something like

[1] I am aware of the objections raised against this term, but do not think that they are relevant in this context.

[2] " Concept of Truth in Formalized Languages ", in *Logic, Semantics, Metamathematics*, 1956, p. 164.

(T′) A-statement-(to-the-effect-)that-*p* is true if, and only if, *p*. (It is by no means the case that all usages of ' true ', even if referring to statements, conform to this criterion, in spite of its utterly trivial appearance. There is, for instance, a quite common usage, not only in philosophical but also in ordinary discourse, under which a statement-that-*p* is regarded as true (-for-N) if the assumption-that-*p* is, in some sense, useful (-for-N), even if not-*p*.)

Truth as applying to statements should not be confused with truth as applying to propositions[1]—where ' proposition ' is meant as synonymous with ' what is expressed by a declarative sentence '; on the contrary, my misgivings about treating truth as a property of sentences (of ordinary language) apply, *mutatis mutandis*, also to treating it as a property of propositions. (Statements are " made ", but neither sentences nor propositions are " made ".)

It is, of course, much simpler to manipulate sentences in logical arguments than the incomparably more elusive and less definite statements. It is indeed quite often perfectly safe to deal not with certain statements but with the sentences that are usually uttered when one wants to make these statements. In scientific matters especially it does little harm and a lot of good to work with sentences and assign them truth-values, so long as it is kept in mind that this is a matter of convenience only and that one must be prepared to return to statements as soon as trouble arises. But this procedure is suicidal when working with context-dependent sentences. Applying logic to context-independent sentences of ordinary language has been on the whole fairly successful; applying logic to context-dependent sentences by treating them as entities that are either true or false must occasionally lead to spectacular failures, as we can now say by hindsight. But the proper conclusion from these failures is not that ordinary language is inconsistent, but that when the application of the rules of logic to ordinary language sentences leads to paradox, one has to fall back on statements. In the case of the Liar at any rate, the paradox disappears; and this holds not only for those versions of the paradox that work with context-dependent sentences, like Kemeny's, but also for such versions as Strull's[2] where the sentences involved are context-independent, as the reader is invited to check by himself.

[1] This conception was once called the " absolute " one by R. Carnap. See *Introduction to Semantics*, 1946, p. 90.

[2] Avrum Strull, " Is Everyday Language Inconsistent? ' *Mind*, Vol. LXIII, 1954, pp. 219–225, especially pp. 224–225.

Even if it could be shown that ordinary language is inconsistent, for reasons other than the occurrence of the Liar paradox, much could be said—much more than was said by Strull—against Kemeny's claim to infer from this the unsuitability of ordinary language as a medium of logical arguments. But I have reasons to suspect that Kemeny and other recent authors who have expressed themselves on this issue in a similar vein (like R. M. Martin[1]) do not really want their claim to be taken both literally and seriously, but rather as an indirect and persuasive appeal for doing philosophy with the assistance of (partly) formalized language systems. And as this is something to which I consent wholeheartedly, I feel that further discussion might conceivably do more harm than good and tend to perpetuate a mainly, if not purely, verbal issue.

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[1] See "Some Comments on Truth and Designation", ANALYSIS, Vol. X, 1950, p. 65, and "On 'Analytic'", *Philosophical Studies*, Vol. III, 1952, p. 44.

GOOD AND EVIL AND MR. GEACH

By A. M. MacIver

THREE cheers for Mr. Geach[1] for showing how much nonsense contemporary moral philosophers might have saved themselves from talking if they had only remembered their Plato and Aristotle. But does he really think that we can just go back to the Fourth Century B.C. and stay there? Things have happened in the intervening twenty-three centuries. Assumptions which Plato and Aristotle made have been proved false, and questions have been formulated to which they gave not even unsatisfactory answers but none at all. To anyone who has ever tried to explain Plato and Aristotle to present-day (particularly Greekless) students it must seem that Mr. Geach has only said what it is easy to say and skipped over all the difficulties. We have probably all of us found that it is comparatively easy to make comprehensible the queer talk about " virtue " in pruning-knives and eyes; what is difficult is to justify the transition from this to what we call " *moral* virtue " —and that because we ourselves, as well as our students, have reason to wonder whether it is in fact justifiable.

I am thinking particularly, because I lecture every year to a first-year General class on Plato's *Republic*, of the passage near the end of the First Book which is one of the classic statements of the connection of *aretê* (" virtue ") with *ergon* (" function ").[2] Whatever has an *ergon* must have the corresponding *aretê* in order to perform it. But *aretê* in classical Greek is nothing but the abstract noun which corresponds to the adjective *agathos* (" good "). What is being asserted is just that anything which has a function is called " good " in proportion as it can perform that function effectively. This is exactly what Mr. Geach says, and what is in question (so far) is the " goodness " of Mr. Geach's " good hygrometer " or " good batting wicket ". But if, whenever " virtue " is ascribed to *men*, all that is meant is *aretê* thus conceived, could such a conception ever lead (except by a jump) to anything more than what some Greeks actually did derive from it—namely, what Meno learnt from Gorgias,[3] that there is a virtue of the freeman, a virtue

[1] P. T. Geach, " Good and Evil ", Analysis 17.2 (December 1956), pp. 33–42.
[2] *Rep.* I, 352d–353d.
[3] Plato, *Meno*, 71e–72a.

of the slave, a virtue of the wife, a virtue of the unmarried girl, and so on, but nothing which is " human virtue " as such? In this sense we can speak of " a good husband ", " a good father ", " a good Chancellor of the Exchequer ", " a good business man ", " a good shoemaker ", but hardly of " a good *man* ".

Mr. Geach allows that there is a difficulty here, but only says of it that he " thinks it could be overcome ".[1] One wishes he had indicated how. I have heard someone—I have quite forgotten who it was, but I am sure he was a philosopher and he is probably a reader of ANALYSIS, so I must apologise to him for quoting without acknowledgment—say that one of his children had asked " Daddy, what are people for? ", but to us that seems a childish question. (It is not the same as the theological question " Why did God create men? ". The purpose for which a thing is made need not be its function. Hygrometers are, I suppose, usually made *for sale*, and even if the manufacturer expects his products normally to be bought by those who want to use them, it would be all one to him if they sold well only because there happened to be a fashionable craze for collecting hygrometers. Things can be made for sale, and for other reasons, which have no functions, and God could have had reasons for creating men without functions.)

Aristotle, who saw that this step had to be taken, seems to have seen no way to justify it logically, for he resorts to rhetorical devices to put it across:

> " Just as for a flute-player, a sculptor, or any artist, and, in general, for all things that have a function or activity, the good and the ' well ' is thought to reside in the function, so it would seem to be for man, if he has a function. Have the carpenter, then, and the tanner certain functions or activities, and has man none? Is he born without a function? Or as eye, hand, foot and in general each of the parts evidently has a function, may one lay it down that man similarly has a function apart from all these? What then can this be? "[2]

He had behind him the assumption, which it looks as if Plato had inherited from Socrates, that all definition must ultimately be teleological—that to show what a thing *is for* is the only way to explain what it *is*. But we now, even if we might allow that this is true of some things (such as hygrometers and batting

[1] *loc. cit.*, p. 39.
[2] *Eth. Nic.* I. 7, 1097b 25–33 (Ross's translation).

wickets), would say that it was false of others (such as pebbles and volcanoes). Talking, as most of us nowadays prefer to talk, about the use of *general names*, we should say that some of these, such as the general name " hygrometer ", involve a reference to a function, so that nobody could use the word " hygrometer " correctly without saying something about the purpose for which anything to which the name was applied could be used, from which it follows that anyone who knows what the word " hygrometer " means must know what the expression " good hygrometer " means, but that the same is not true of other general names, so that knowing how to use the word " pebble " does not involve attaching any sense to the expression " a good pebble ". And we might allow that knowing how to use the words " carpenter " and " tanner " involved knowing what must be meant by " a good carpenter " or " a good tanner ", without concluding that to know how to apply the word " man " must involve knowing how to apply the expression " a good man ".

At this point Mr. Geach seems to me to lose his way. At least, I begin here to find him either very difficult to follow or quite unconvincing. Unlike Plato and Aristotle he assumes (in the regular modern fashion) that the question at issue must be what is " a good *human action* ", not what is " a good *man* ". If he intended to develop his argument in this way, I think he ought to have used different examples earlier. He ought to have considered, not what is meant by " a good hygrometer ", but what would be meant by " a good hygrometric reading ". But this might have been done, though it sounds odd, and perhaps so far the amendment would not make much difference. But, if I don't find it a very plausible account of what I mean by " a morally good man " that it is a man who can effectively perform the functions of a man as such (whatever those may be), I find it an even less plausible account of what I mean by " a morally good human action " that it is an action which performs the function of a human action as such. Plato and Aristotle, I am sure, never said anything so questionable. Plato in the *Republic* (in effect) defines " a just *soul* " as " a *well-ordered* soul ", but not " a just *action* " as " a well-ordered *action* ". It is implied that the just action is sufficiently identified as the sort of action that a man with a well-ordered soul would perform. And so also with Aristotle in the *Ethics*. It is not the moralist's function, according to either Plato or Aristotle, to tell you what you ought to *do* in any circumstances; nobody, according to them, can

tell you that, but you will see it for yourself, as soon as you are in the circumstances, if you are a good man; what the moralist can tell you is what will make you good.

But Mr. Geach is so far Kantian that he is sure the moral philosopher is concerned with *duties*. Therefore he himself has to raise the question, by what logical step we can pass from "Adultery is a bad human action", understood as he interprets it, to "You ought not to commit adultery".[1] His answer is that, to any question of the form "why should I?" or "why shouldn't I?", "the only relevant answer is an appeal to something the questioner *wants*". And it is true that the distinction of "good" and "bad", in Mr. Geach's sense, is of no importance unless things are wanted. If "a good hygrometer" is defined as "an instrument with which it is easy to measure the humidity of the atmosphere", this conception is of merely formal interest unless someone at some time actually *wants* to measure the humidity of the atmosphere. But Aristotle saw—though Mr. Geach, apparently, does not—that merely being wanted is not enough. Even if people *do* want to measure atmospheric humidity, the question can still be raised whether this *has any point* or is only (say) a crazy fashion or a foolish tradition. And it is just not the case that everyone actually wants either to perform "good human actions" or to achieve anything to which performing them is instrumental. As Aristotle saw, wanting to be a good man is already a mark of being one. The appeal must be, not to what a man *does* want, but to what he *would* want *if* he was a *good* man. It is only for the virtuous man (the *spoudaios*) that "the true good" and "the apparent good" coincide, and, though it is a proper object of "prayer" or "pious hope" (*eukhê*) that what is "good absolutely" should also be "good for us", each of us has to pursue what is "good *for him*" (and so, until he becomes virtuous, *cannot* do what he ought).[2] That is to say, the appeal is not to what we *actually* want, but (in modern language) to what we *ought to* want.

But Mr. Geach here is in an embarrassment. He has to assume dogmatically that we *all actually* want to perform "effectively human actions", just because, if this is questioned, he has no reason to offer why any of us *should*. Plato and Aristotle were in no such difficulty, thanks to their teleological conception of "nature". Even "artificial" things (such as knives) are *defined* by a function which they may perform only

[1] *loc. cit.*, p. 39.
[2] *Eth. Nic.* III.4, 1113a 23–31, and V.1, 1124b 5–6.

imperfectly. Anything may be called a " knife " in proportion as it is possible with it to cut anything without cutting one's fingers—which is why even " eoliths " (the sharp flints supposed to have been picked up and used by the most primitive men with little or no further shaping) can already be called " flint knives " —but in this sense some things are " more knives " than others, and it can be said that for Plato the " better " knife means simply the " knifier " knife. But " natural " things not only " are what they are " in different degrees, but have their own active tendency to *become* " what they are " (somewhat as the bent bow has an active tendency to return to the straight as soon as the string is released). In human beings this reveals itself as a kind of *felt dissatisfaction* so far as a man is not yet " effectively human ". (To use the mechanical sort of image which illuminates things to us, though Plato and Aristotle themselves would never have used it, because there was so little mechanism in their environment: the bad man is like a snarled spring, which remains compressed not because it is without elasticity, nor yet because anything external is compressing it, but because it is tangled up in itself.) That the vicious man is as such unhappy is the conclusion of Plato's *Republic* and the starting-point of Aristotle's *Ethics*. There is an answer to the question what Aristotle's " good man " is " good *at* ", and it is that he is " good at *being happy* ". That is how Aristotle's account of " virtue " fits into a work which begins and ends as an account of " human happiness ".

I may be wrong but I suspect that Mr. Geach is trying to be an Aristotelian by bits and pieces, appropriating Aristotle's account of " goodness " but discarding his teleological physics and metaphysics. I very much doubt whether this can be done. The Aristotelian system more than any other (except perhaps the Hegelian) hangs together, so that, if you accept any part of it, you are liable to find yourself committed to maintaining that the earth is at the centre of a finite spherical universe and the heavenly bodies revolve in perfect circles.

Let us suppose, however—falsely, I am sure—that Mr. Geach is ready to jettison the achievements of two thousand years of science. Is what Aristotle gives us a system of what *we* mean by " morality "? Was it even meant to be? I am sure that for Mr. Geach it is a *datum* that to commit adultery is *wrong*. Philosophically he is only interested in the question why this is so and what it involves. But for Plato and Aristotle it was a *question* whether the sort of man who commits adultery is *happy*. The conclusion to which they came was that he was

not, and they certainly considered it important—a confirmation, if that was needed, of the validity of their argument—that the way of life which philosophy proved wise was also traditionally respectable. If, however, it had not been, they would have still followed the argument whithersoever it led, as did philosophers of other schools, such as the Cynics and Cyrenaics, to whom philosophy seemed to call for an abandonment of conventional morality.

The fact is that what Plato and Aristotle meant by " moral virtue " was much more like what a psychiatrist means by " mental health " than like any of the things which modern moral philosophers talk about. It is that without which a man cannot be satisfied, whatever good fortune his life may bring him. The division among the ancient moralists parallels that among the psycho-analysts between those who (like Freud) seem confident that the existing social order is psychologically all right, so that a man who was mentally healthy would be found behaving in ways considered laudable by his neighbours—sticking to his one lawfully wedded wife and so on—and those who (like Erich Fromm) allow that a whole society can be " neurotic ", so that a healthy way of life might be one that was not socially approved.

I am myself more ready than I fancy Mr. Geach would be to take the step which would make sense of his argument as a whole, which would be to scrap the whole system of " morality " as generally understood—all notions of " right " and " wrong "—as a bad social tradition. (It certainly does not seem to me *self-evident* that the sense of moral obligation deserves any more respect than a neurotic compulsion: that is something which needs to be demonstrated, or at least shown to be reasonably probable.) This would put us back where the Sophists and Socrates started to build ancient " moral philosophy " on the ruins of the customary taboos and tribal loyalties which had been the " morality " of the pre-historic Greeks. But, if we take such a drastic step, we ought to know what we are doing and not think we are doing something else.

At the end of Mr. Geach's article I must confess to a suspicion that he has fallen into the commonest of all philosophical pitfalls—that of giving a term a peculiar sense of his own and then himself understanding it as commonly used, not as his own definition requires. He talks of " good human actions " and himself forgets that he does not mean by this what anyone else would mean—namely, " *right* actions ". But all that he is

entitled to mean, according to his own account, is actions which completely satisfy the specific purposes of human beings, whatever these may be found to be. That is to say, he thinks that he is defending " morality " when (if anything) he should be undermining it.

University of Southampton.

CONTRARY-TO-FACT CONDITIONALS AND LOGICAL IMPOSSIBILITY

By ALAN R. WHITE

MR. JOHN WATLING recently[1] advanced an argument about contrary-to-fact conditionals which he summarised as follows. " The truth of a contrary-to-fact conditional is not determined by the truths of its components. The truth of a conditional that is not contrary-to-fact *is* determined by the truths of its components. But it is logically possible that a conditional that is contrary-to-fact should not be contrary-to-fact. Therefore it is logically possible that the truth of a contrary-to-fact conditional should be determined by those of its components." (p. 77.) I shall try to show that there is a mistake in this argument. Consider, for this purpose, the following two arguments which seem to me to be analogous to it.

(a) The truth of an analytic statement is not determined by the state of the world. The truth of a statement which is not analytic, that is, which is empirical, *is* determined by the state of the world. But it is logically possible that a statement which is analytic should not be analytic. Therefore it is logically possible that the truth of an analytic statement should be determined by the state of the world. And this, I think, is manifestly mistaken.

It might, however, be objected that the premiss that it is logically possible that a statement which is analytic should not be analytic is in fact false. If I offered the statement " Phosphorus melts at 44° C." as an example which has been said to be at one time an analytic and at another time not an analytic statement, it might be replied that this is only an example of a *sentence*, a set of words, which at one time was used to express an analytic statement and at another to express a non-analytic statement. Since Watling uses " conditional " in such a way as to speak both of the truth and of the meaning of a conditional, I doubt whether this sort of objection is open to him; but as I have myself much sympathy with the objection, I shall concentrate on the second of my two analogous arguments.

(b) The wealth of an unmarried man is not affected by the expenditure of a wife. The wealth of a married man *is* affected

[1] ANALYSIS 17.4 (March 1957), pp. 73–80.

by the expenditure of a wife. But it is logically possible that a man who is unmarried should be married. Therefore it is logically possible that the wealth of an unmarried man should be affected by the expenditure of a wife. And is this not clearly mistaken?

The source of the mistake lies, I think, in something which Watling says on the next page (p. 78). He there says that "A statement logically implies that its negation is false, but it does not logically imply that its negation is logically impossible." This is true. But if the statement in question is an analytic one, then it does follow that its negation is logically impossible. For instance, the (empirical) statement that this man is married entails that it is false that he is not married but not that it is logically impossible that he be not married. But the statement that a husband is married, being analytic, is necessarily true; and this entails that it is logically impossible that a husband be not married. Now, the statement that this is a contrary-to-fact conditional is an empirical statement and thus entails that it is false that this is not a contrary-to-fact conditional, but not that it is logically impossible that it should not be; whereas the statement that the truth of a contrary-to-fact conditional is not determined by the truths of its components is an analytic statement about contrary-to-fact conditionals, and its being, as such, necessarily true entails that its negation is logically impossible.

If anyone were to reply that the statement " The truth of a contrary-to-fact conditional is not determined by the truths of its components " is only an empirical statement, then Watling's desired conclusion might follow. But there would no longer be any need for his elaborate argument to prove it, since it follows simply from the alleged fact that this statement is empirical. That is, to say that this statement is empirical *is* to say that it is logically possible that the truth of a contrary-to-fact conditional should be determined by the truths of its components; and this is his conclusion.

But what evidence would anyone give to show that the statement " The truth of a contrary-to-fact conditional is not determined by the truths of its components " is empirical? This is certainly not shown by the true premiss " It is logically possible that a conditional which is contrary-to-fact should not be contrary-to-fact ". For, although it is logically possible that a man who is married should not be married, this does not show that the statement " The wealth of a man who is unmarried is not affected by the expenditure of a wife " is empirical. There

are many empirical statements about unmarried men and about contrary-to-fact conditionals, but the mere truth that it is logically possible that a man who is unmarried should be married or that a conditional which is contrary-to-fact should not be contrary-to-fact does not enable us in any way to decide which statements about unmarried men and contrary-to-fact conditionals are empirical and which are analytic. What Watling has " proved " is the obviously true statement that what is true of a man when he is married or of a conditional when it is contrary-to-fact is not necessarily true of the man when he is not married or of the conditional when it is not contrary-to-fact, and *vice versa*. He has not proved anything about what are the characteristics of a man when he is married or of a conditional when it is contrary-to-fact, not even what are their logically possible characteristics. He may have overlooked this because of a fallacy of equivocation lurking in the pronoun " it " in the following sentence. " That a conditional is contrary-to-fact entails that it is not in accordance with fact, but it does not entail that it is logically impossible that *it* should be in accordance with fact " (p. 78, my italics). If the italicised " it " refers to " conditional ", this is quite true but irrelevant, for it shows nothing about " contrary-to-fact conditionals "; whereas if it refers to " contrary-to-fact conditional ", what is said is relevant but false. It is not logically impossible that a *man* should not be married, but it is logically impossible that a *husband* should not be married.

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NAMES OF WORDS: A REPLY TO DR. WHITELEY

By G. E. M. ANSCOMBE

DR. WHITELEY has produced[1] a solution of the paradox about names of words modelled upon Wittgenstein's remark

> When I say to someone "Pronounce the word 'the'", ... the second "the" . . . . has a role just like that of a colour-sample . . . .; that is, it is a sample of what the other is meant to say.

I said that this solution is a clear one only if its application is very restricted, as it is in this passage, and that it is not clear how it would be applied to e.g. "'Red' is the name of a colour". Dr. Whiteley in effect says that it is clear in such cases.

He says "I have not named the name; I have produced the name itself; more precisely, I have produced a token of the type 'Smith'". But I do that, surely, every time I use the name to say something about Smith?

The rest of his solution could I think be paraphrased as follows: to write "'red' is the name of a colour" is in effect the same as to write down this :

red

"The shape in the box is the name of a colour."

Such a statement would be clear, I think, if the sentence under the box had been e.g. "The shape in the box is easily copied." But since I shall discover the shape in the box in the word "coloured" or "redoubtable", the way this works for "'red' is the name of a colour" is not clear.

Suppose Dr. Whiteley says: just as, when I shew someone a colour sample and say "This is the colour I painted my house", he has to know how to take the sample—e.g. disregarding its shape—so in this case, he has to know how to take the sample. And he *does* know if he knows English, because he understands "is the name of", just as in the other case he understands "is the colour of".

[1] *Analysis* 17.5 (April 1957) p. 119.

I reply: in the case of the colour sample, the sample enables him to identify any colour which is the same as it, as *the* colour the house is painted; but the pattern of the written word does not identify anything that is equiform with it, as *the* word. For there is such a thing as equivocation, as well as the possibility of the pattern's occurring non-significantly as part of another word. The latter point only seems unreal because printers divide words up for us, as ancient MSS used not to do: but the division is consequent upon the actual distinctness of the word and not *vice versa*. Anyway the pattern *is* the name of a colour in " redhead ".

If you memorize the pattern of a word in a foreign language, this is certainly an aid to identifying the word; but it is only in context that you can identify the word with confidence. The pattern in the box is *ex hypothesi* out of context. So there it is not the word; hence it is *not* the name of a colour. But " ' red ' is the name of a colour " is true; therefore the pattern as it occurs in that sentence is not occurring as it occurs in the box.

It appears to me that the solution must be that in this use the word refers to its uses in other sentences, and that the expression " is the name of " indicates that it has this special use here. There is thus a certain justification for speaking of a word as *named* in such sentences; for a use (which constitutes the word as that word) is being referred to. But this solution is not perfectly clear: for if I say " in this use the word refers to *its* uses in other sentences ", I can surely be asked to explain what I mean by the " it " whose uses in other sentences are being referred to: and for the reasons I have given I do not think it right to reply that " it " is the pattern; it is the word itself.

Someone who understands the sentence " ' red ' is the name of a colour " first and foremost understands the predicate ". . . . is the name of a colour " and if he understands a word as filling up the blank space, then he understands it as a word constituted by a certain use in other sentences.

It will be asked: if this is correct how can " ' X ' is the name of a colour " have informative force, as it undoubtedly has?— It is curious that, supposing it to be true, a more *specific* piece of information which actually tells us which colour " X " is the name of, seems rather lacking in informative force: " ' X ' is the name of X ". It is however not totally lacking in informative force, as can be seen by the fact that there are restrictions on what can be substituted for X : " ' Of ' is the name of of "

and " ' Someone ' is the name of someone " obviously being pseudo-sentences that arise from making incorrect substitutions.

Dr. Whiteley is obviously right in thinking that the informative force of sentences of this type *is* connected with the pattern. The sentence " ' red ' is the name of a colour ", so far as it is informative, does tell us that the pattern of the subject word occurs as a word with a certain use in other sentences. It is for this reason that a person might in suitable circumstances glean false information from the pseudo-sentence " ' Someone ' is the name of someone ": namely, that the pattern of the ostensible subject-word was someone's name; and that is the reason why we are inclined to say that this is a real false sentence and not just a pseudo-sentence.

If anyone thinks it absurd of me to call " ' Someone ' is the name of someone " a pseudo-sentence and not *just* a false one, he should consider that if it is false, it might have been true. So let us suppose it is true: someone christens his child " Someone ". In that case *a* sentence " Someone ' is the name of someone " will have become true; but this is not because what we *now* intend if we say ' It is not the case that " Someone " is the name of someone' has become false.

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DESCRIPTIONS, SCOPE AND IDENTITY

By HERBERT HOCHBERG

IN *Principia Mathematica* Whitehead and Russell (A) define '$x=y$' as '$(\phi):\phi x.\supset.\phi y$'[1]; (B) treat (1) '$(\iota x)(\phi x)=(\iota x)(\phi x)$' as analytically equivalent to (2) '$(\exists b):\phi x.\equiv_x.x=b:b=b$'; and (C) adopt as a notational convention the omission of a scope operator for a description when the scope intended is the smallest sentence in which the description in question occurs. However, the definition in (A) cannot be applied to the '$=$' of (1), since descriptions cannot be treated as arguments, without an additional premiss. This prevents us from arriving at (3) '$(\iota x)(\phi x)=(\iota x)(\phi x).\equiv::(\psi):\psi(\iota x)(\phi x).\supset\psi(\iota x)(\phi x)$' where the left hand side of the equivalence is not, by (B), analytic, while the right hand side is, by (C). Since both '$=$' and the incomplete symbol '$(\iota x)(\phi x)$' are defined signs in *PM*, we may conclude that the order of replacement of defined signs makes a difference or, even, that the definition of '$=$' and the "rules" for handling descriptions *require* a specific order of replacement. Or, since '$=$' is not directly replaceable in contexts involving descriptions, one might even suggest that it operates, in some sense, as an undefined sign in those contexts.

If, however, we forget (C) and write (1) as (1′) '$[(\iota x)(\phi x)].(\iota x)(\phi x)=(\iota x)(\phi x)$', we note two things: (a) the identity sign in (1′) is now directly replaceable without giving rise to (3), and, consequently, (b) the order of replacement in (1′) no longer matters. For, replacing '$=$' in (1′), assuming for the moment that we may, we get (4) '$[(\iota x)(\phi x)]::(\psi):\psi(\iota x)(\phi x).\supset.\psi(\iota x)(\phi x)$', from which, by expanding the description, one gets (5) '$(\exists b)::\phi x.\equiv_x.x=b:.(\psi):\psi b.\supset.\psi b$'. (5) is not analytic, and, moreover, it is just what one gets by expanding the description *before* the identity sign in (1′). Hence, instead of (3), one can now only prove the equivalence of (1′) and (4). The use of the scope operator preserves the "existence condition" that prevents (3). Thus, if we abandon the notational convention stated in (C), we may be said to have, due to (a) and (b), a more general treatment of identity, since we can now afford to alter (A) in order to allow '$=$' to be directly replaceable in contexts

[1] I omit '$!$' from the definition (of '$=$') where that sign is supposed to indicate "predicative contexts", *i.e.*, the defining phrase, as it occurs in *PM*, is '$(\phi):\phi!x.\supset.\phi!y$'.

involving descriptions. A rather minor alteration in the conventions of *PM* thus results, I believe, in an improvement in the theory of descriptions presented there.

Mr. Geach has pointed out[1] that in the case of a context like (1) the Russell-Whitehead definition of '$[(\iota x)(\phi x)] \,.\, \psi(\iota x)(\phi x)$' by '$(\exists b) : \phi x \,.\equiv_x .\, x = b : \psi b$' leads to an ambiguity. For, it is not determined which of

$$(a_1)\quad b = (\iota x)(\phi x)$$
$$(a_2)\quad (\iota x)(\phi x) = b$$
$$(a_3)\quad b = b$$

would replace 'ψb'. Mr. Geach argues that (C) does not prevent this ambiguity since (1) contains no part that is a sentence. Actually an analogue of Mr. Geach's point can be raised about contexts that do contain sentences as parts. Consider (4). It is analytically equivalent to (5), but we could consider typographically different expressions as its expansion. (5) would be one of these. Another would be (5′) '$(\exists b) :: \phi x \,.\equiv_x .\, x = b :: (\exists c) :. \phi x \,.\equiv_x .\, x = c :. (\psi)\, \psi b \,.\supset .\, \psi c$'. The point is simply that the *PM* conventions are stipulated for descriptions rather than for *occurrences* of descriptions. Thus, where more than one occurrence of the same description falls under a scope operator we face the ambiguity in question. In keeping with the illustrations, (5) and (5′), we can see two alternative solutions.

First, taking a hint from the treatment of '$(\iota x)(\phi x) = (\iota x)(\psi x)$' in *PM*, where the description occurring first typographically is to have the larger scope, we may introduce the following convention. (I) Where two or more occurrences of a description are *bound* by an occurrence of a scope operator then the first occurrence (typographically) has the larger scope, the second occurrence the next largest scope, etc. In stating (I) I used the term 'bound'. This is to prevent a problem arising from an occurrence of a description which lies within the scopes of two occurrences of the same scope operator. For example, consider '$[(\iota x)(\phi x)] :. \psi(\iota x)(\phi x) \,.\supset : [(\iota x)(\phi x)] \,.\, \psi(\iota x)(\phi x)$.' The scope of the occurrence of '$(\iota x)(\phi x)$' in the clause to the right of the conditional is intended to be determined by the second occurrence of the scope operator and not by the first. We must thus indicate which occurrence of the description is "determined" by which occurrence of the scope operator. Since speaking in terms of occurrences of descriptions and scope

[1] P. T. Geach, "Russell's Theory of Descriptions", ANALYSIS, 10.4, March, 1950.

operators bears an evident analogy to quantified variables and quantifiers,[1] the term ' bound ' is suggestive. An occurrence, α, of a description will be said to be bound by an occurrence, β, of a scope operator if:

(b_1) α lies within the scope of β;

(b_2) β contains an occurrence of the description;

(b_3) α does not lie within the scope of another occurrence, β', of the scope operator such that β' lies within the scope of β. We may notice that in accordance with the proposal to drop (C) no well-formed expression would contain a " free " description.

According to (I), (5′) is the result of replacing the descriptions (the two occurrences of the same description) or the identity sign first in (1′). The same result is achieved even if one replaces one occurrence first, the identity sign second, and the other occurrence last. In this latter case one simply has to keep an occurrence of a scope operator to indicate the scope of the remaining occurrence of the description. Likewise, (I) specifies the replacement of ' ψb ' by (a_1) in Mr. Geach's illustration.

A second and typographically simpler alternative is to adopt the following convention. (II) ' $[(\iota x)(\phi x)] \,.\, \psi(\iota x)(\phi x)$ ' is defined by ' $(\exists b) : \phi x \,.\equiv_x .\, x = b : \psi b$ ' where (with ' β ' standing for the occurrence of the scope operator in the definiendum) ' ψb ' is like ' $\psi(\iota x)(\phi x)$ ' except for containing a free occurrence of ' b ' at each place that ' $[(\iota x)(\phi x)] \,.\, \psi(\iota x)(\phi x)$ ' contains an occurrence of ' $(\iota x)(\phi x)$ ' bound by β.[2] By (II), (5) is the result of replacing either the descriptions (occurrences) or the identity sign first in (1′). In this case, however, both occurrences must be replaced simultaneously. (II) likewise provides for an unambiguous replacement of ' ψb ' by (a_3) in Mr. Geach's illustration.

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[1] Compare W. V. Quine, *Mathematical Logic* (1951), pp. 74–8.

[2] (II), as well as (I), must also require that replacing descriptions does not involve one in problems (similar to those occurring in illicit substitutions) by permitting an introduced quantifier to " catch " free variables. See Quine, *Methods of Logic* (1950), pp. 133–4.

ON BELIEFS ABOUT ONESELF

By P. T. Geach

If we are attempting a formal treatment of certain problems—*e.g.* Moore's paradox about saying and disbelieving, or the question what it is for one assertion to commit somebody to another assertion—we may well find ourselves using the symbolism " x believes that Fx ", or something essentially like this. Now there are difficulties here; what are we to take as substitution-instances of " x believes that Fx "? Can we, in the first place, obtain substitution-instances of it by writing an actual predicate for the " F " and a designation of a person for both occurrences of " x "? This will hardly do. For, if the logic of " x believes that Fx " is not to be intolerably complicated, we want the following ' Leibniz ' law of identity to hold as a logical truth:

(A) If x is the same person as y, and x believes that Fx, then y believes that Fy.

Now of course we can assert as a logical truth:

(B) If Philip's worst enemy is the same person as Philip, and Philip's worst enemy believes that Philip's worst enemy is dead, then Philip believes that Philip's worst enemy is dead.

But we certainly cannot assert as a logical truth what would, on our present assumption, be a substitution-instance of (A):

(C) If Philip's worst enemy is the same person as Philip, and Philip's worst enemy believes that Philip's worst enemy is dead, then Philip believes that Philip is dead.

(C) is obtained from (B) by substituting " Philip " for " Philip's worst enemy " in the consequent; although the antecedent contains the conditional clause " Philip's worst enemy is the same person as Philip ", this substitution is not warranted thereby, since it is made within *oratio obliqua*.

We could, however, maintain the logical truth of (A) by a different reading of " x believes that Fx ", according to which a substitution-instance of " x believes that Fx " would be, not " Philip's worst enemy believes that Philip's worst enemy is dead " but rather " Philip's worst enemy believes that *he himself* is dead "—" he himself " being an *oratio obliqua* proxy for the first-person pronoun of *oratio recta*. For now a substitution-instance of (A) would be *e.g.*:

(D) If Philip's worst enemy is the same person as Philip, and Philip's worst enemy believes that he himself is clever, then Philip believes that he himself is clever.

And this is indeed logically true.

This solution requires that it should be legitimate to treat "—believes that he himself is clever" as a definite, univocal predicate, no matter what subject we attach it to. I have myself no objection to such predicates, containing what classical grammarians call an indirect reflexive pronoun; though those who think there is something fishy even about predicates containing direct reflexive pronouns, like "— is divisible only by itself and unity", will *a fortiori* suspect these. But if we say of a number of people that each of them believes that he himself is clever, what belief exactly are we attributing to all of them? Certainly they do not all believe the same proposition, as "proposition" is commonly understood by philosophers. I hope this note may provoke more discussion of this class of predicates.

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ERRATUM

In Vol. 17 No. 6 (June 1957), page 131, 4th line from foot of page, for

$$\mathfrak{T}\phi_G\mathfrak{U}_p$$

read

$$T\phi_G\mathfrak{U}_p$$